THE

PSYCHOLOGICAL BULLETIN

THE ILLUSION OF CLEAR VISION DURING EYE MOVEMENT.[1]

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Some time ago the PSYCHOLOGICAL REVIEW published a short contribution to the theory of the function of the eye movements under the title, ' Visual Perception during Eye Movement.' Possibly ' The absence of visual perception during eye movement ' would have been a more appropriate title, since the paper maintained that under ordinary circumstances no new visual impressions are received during eye movements of the simple reaction type. The evidence was clear enough. The only really surprising fact was that it should have needed expression at all. It would seem as though rapidly recurring moments of practical blindness to events in the environment ought to need no detailed proof. That the matter did require proof and that this was received with some scepticism created a new problem in psychological optics, viz., the cause of the illusion of continuous clear vision during eye movements of the simple reaction type.

The first systematic attempt to answer this problem and the allied problem why we do not see the fusion of the visual field, that theoretically might be expected to accompany the rapid succession of stimuli during eye movement, was a paper by E. B. Holt in *Harvara Psychological Studies*, Vol. I. This paper reaches the conclusion that ' voluntary movements of the eyes condition a momentary visual central anæsthesia.'

While I have not been able to reach entire agreement with this conclusion, it seems to me that we must look in the direction Mr. Holt has pointed out for any final solution of the problems. How-

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[1] Read in part at the meeting of the New York Academy of Sciences in New Haven, March 27, under the title 'Central Anæsthesia During Eye Movement.'

ever, if I might state my conclusion at the beginning, I should want to substitute the words inhibitory process for anæsthesia in his formulation.

With regard to the causes of the inhibitory processes my disagreement with the Harvard Studies is more radical. Mr. Holt conjectures that eye-movement anæsthesia is the effect of 'muscle sensations of present movement streaming to consciousness to form the basis of the new post-motum localization.' Without admitting the efficacy of such sensations of eye movements to produce a visual anæsthesia I must protest again that I do not find in myself sensations of eye movement 'streaming to consciousness'; and moreover, that a careful examination of a considerable number of persons during the last ten years makes it clear that in this respect I am not altogether abnormal.

But even if eye movement sensations were 'streaming' to consciousness, anæsthesia would be a rather unprecedented consequence. It does not occur in touch as a result of moving the hand; neither does it occur in the slower and more easily observable kinds of eye movement. When we follow a moving object, for example, eye movement is a condition of clear vision, not a condition of anæsthesia. Similarly, when the fixation of an object is maintained during movements of the head, by means of compensatory eye movements, neither head movements, nor eye movements, nor both together produce anæsthesia. Obviously these facts do not disprove Mr. Holt's hypothesis with respect to the more rapid forms of eye movement. But they certainly do create a certain presumption against it; and we have a right to demand as a condition of proof unequivocal evidence. Unequivocal evidence, however, is not at hand.

The main experimental data on which the hypothesis of anæsthesia during eye movement depends are comparisons of the perception of concrete objects stimulating, under similar circumstances, the moving and the motionless eye. The plan is irreproachable, but unhappily the maintenance of similar conditions in the two instances is extremely difficult. It would be impracticable to attempt a complete discussion of Mr. Holt's experiments at this time, but I hope I shall not appear ungrateful for the services he has rendered the problem if I call attention briefly to some questionable phases of his argument.

1. The pendulum tests, the only experimental support for his hypothesis, give results which can be interpreted as favoring central anæsthesia only occasionally. Otherwise, they are either indifferent, or in some cases, at least, distinctly opposed to that hypothesis.

2. All the pendulum tests presuppose experimental conditions

which I have found it impossible to duplicate either for myself or for my pupils. A voluntary reduction of the velocity of the simple reaction movements of the eyes to 10° in 120 σ, or anything approximating that velocity, without interrupting the movement has been consistently impossible. Undoubtedly some such velocity is momentarily approximated at the very beginning and at the very end of any simple reaction movement. Unquestionably some of the phenomena Holt describes would be easily accounted for on such a basis, but that interpretation is I think rather arbitrarily denied us by the author.

3. Velocities such as the above correspond closely either to simple pursuit movements, during which, as we have seen, vision is notoriously good; or to very short movements of two or three degrees. All the phenomena on which the hypothesis rests may be explained without recourse to central anæsthesia on the assumption of discontinuous sweeps of the eyes, interrupted by these short movements. These are certainly not precluded by Holt's experimental conditions. Moreover, they are very common, and may occur without revealing themselves in any way to introspective analysis.

Let me illustrate: In Holt's first pendulum experiment a dumbbell shaped opening, cut vertically in a screen attached to a pendulum, oscillated between the eye and an illuminated surface, shaped like a very broad dumb-bell, almost like a capital letter H laid on its side. The observer tried to make his eye movements approximate the angle velocity of the small dumb-bell, seeing it, however, only when it was passing the illuminated figure. Occasionally the small dumb-bell was seen complete, occasionally a fainter image of the broad illuminated dumb-bell was seen and occasionally the two ends of the small dumb-bell were seen without any handle. The first phenomenon would naturally occur if the velocity of the eye exactly coincided with the angle velocity of the pendulum at the middle of the exposure. The second would occur if the eye were accidentally at rest during the exposure, as Holt points out. A similar appearance would also occur if there was a gross error in the correspondence of the two velocities. The last appearance, *i. e.*, the handleless dumb-bell, is regarded as the proof of central anæsthesia; on the principle that the difference in the time of exposure of the handle and ends, due to the broadness of the illuminated figure, while not sufficient to render the handle invisible to the eye at rest, is enough to render it invisible to the eye when it is exposed for the same length of time at the middle of an uninterrupted eye movement. But as I have just pointed out there are other conditions involved which would explain the phenomenon with-

out recourse to unusual hypotheses. The handleless dumb-bell would appear if the velocity of the eye happened to coincide with the angle velocity of the pendulum when the dumb-bell shaped opening was passing some other than the central part of the illuminated figure. Or again, it must appear if the illumination was near the threshold and the angle velocities nearly but not exactly coincided. Under these circumstances the total illumination of the handle would be much reduced, and it would consequently be invisible, while the ends might still overlap sufficiently to appear above the threshold and approximately in normal form. Analogous possibilities occur in the second pendulum test with colors.

In view of the ambiguity of Holt's experiments I devised a combination of my perforated disk and his pendulum exposure apparatus. It is well known that the sectors of a rapidly rotating disk, which appear fused to the motionless eye, will flash out as distinct when the eye moves across the disk in the direction of its rotation, at the same angle velocity. Now it is possible to so balance the light reflected from and transmitted through a revolving perforated disk that the perforations shall be just visible under the most favorable conditions of eye movement, but entirely invisible when the eye is at rest. If the same relative illumination be maintained and the same slit be exposed to the motionless eye, the necessary duration of exposure in the latter case compared with the known time of eye movement in the former case will give a fair measure of the degree of visual anæsthesia during eye movement.

These experiments gave entirely negative results for all three of my subjects. Under conditions of illumination such that the slits were occasionally visible, when the rate of movement of the disk precluded their visibility for more than 30 σ, the threshold duration of the exposure of the slit for the motionless eye was approximately 25 σ. This seems to me rather decisive evidence against the hypothesis of central anæsthesia.

Mr. Holt scored against a similar experiment with the objection that there is no evidence the slits are seen during actual eye movement. 'As far as introspection goes,' Mr. Holt contends, 'the retinal impressions may lie dormant until after the moment of eye movement blindness and then flash out as positive after images.' In so far I thoroughly agree with Mr. Holt that there is no introspective evidence in the matter. There is evidence, however, that the perception must occur largely if not wholly after the eye has come to rest, not on account of central anæsthesia during eye movement, but because

of the latent time of retinal inertia and the transmission of nervous impulses.

In short, I have yet to meet with any unambiguous evidence of anæsthesia during eye movement, either central or peripheral, while all my evidence at first hand is negative. Certainly we must all agree that the lack of clear perception during eye movements must rest largely if not wholly on other grounds. Some of these at least are not far to seek. First, I believe we can demonstrate the influence of certain factors usually held to be retinal in their character ; and secondly, I believe we must admit, at least in the longer eye movements, evidence of important central factors.

Chief in the first group is to be reckoned the after effects of stimulation. These alone would account for the nonperception of a fused visual field during short eye movements. An eye movement of 5° occupies about 30 σ, but the positive after image of a moderate stimulation may last 30 σ without perceptible diminution of intensity. A second and probably equally important peripheral factor consists in the inhibitive action of the new stimulation at each new point of regard. These two facts obviously work together to prevent perception of the faint fusion stimulus during short eye movements. The after effect of the first fixation lasts over, in practically its full intensity, until its successor becomes operative. A simple demonstration of these two factors is possible with a falling screen. Between a primary white fixation field and a secondary black one, a band of gray may be exposed for from 25 σ–50 σ without giving any indication of its presence. Similar facts may be observed if colored fields are used, though the threshold duration of exposure differs even more widely than it does in the case of grays.

Even in this experiment the influence of a new factor is indicated. There is usually visible a clearing up of the secondary field. The black gradually grows to its fullest intensity, and the influence of the intermediate color stimulation is often felt indirectly in some modification of the character of this clearing up process. Analogous phenomena may be observed in normal vision. If one looks from a field of some brightness to one of less illumination, some thirty to forty degrees distant, it may be possible to observe a true positive after image of the former field on the latter. More commonly it will be observed that the second field clears up after a perceptible interval as though a veil were rapidly lifted. This appearance may be and often has been referred to the well-known processes of accommodation or the less known facts of binocular incoördination. But the same phenomenon

occurs in monocular vision on a perimeter. Similar appearances may be observed when the line of regard passes across an illuminated field between two darker fixation fields. Even when the light or color differences are insufficient to yield any recognizable separate perception from eye movement, they are often sufficient to perceptibly color the clearing up veil. The undifferentiated effect of adequate stimulation during eye movement is in this case falsely localized at the point of ultimate fixation; and there, like the more familiar obstructions to clear vision due to binocular incoördination and accommodation with which it is so easily confused, it is simply ignored. One of the simplest demonstrations of the clearing up interval is given by negation in monocular vision during eye movements of the utmost rapidity away from and returning to some fixed point of regard. Of course the eye must come to a full stop before it can return; but the return may be begun so quickly that nothing is seen clearly from the beginning of the movement to the end. The veiled appearance is marked and there is no time for the veil to lift.

This process of false localization and abstraction is presumably a central process, but it is a matter of elaboration rather than of anæsthesia. Peripheral conditions indicate why vision during eye movement of the first type cannot be other than confused under ordinary circumstances; and it is in complete accord with other facts of perception that a recurrent episode of disturbance of vision should be ignored. The fencing mask, the muscæ volitantes, and involuntary winking, are illustrations to the point. The real objects of interest in the visual field are the present object of fixation and that peripheral object which is about to become, at the completion of an eye movement, the new object of fixation. Other parts of the field are of lesser attention value, otherwise they would be substituted for the object of fixation. During moments of clearest vision, then, the space about to be traversed by the line of regard during an eye movement of the first type is without especial interest to us; and it is reasonable to suppose that what is without especial interest during clear vision, when the eye is at rest, would arouse no new interest during the confused stimulation incident to eye movements of the first type.

The case is quite different when, as in the Lamansky experiment for measuring the velocity of the eye movements, or in my own perimeter test for vision during eye movement, the real object of interest appears only during eye movement, but in such cases the special training needed for satisfactory experimentation is in itself evidence of new experimental conditions of the attention. Something of the same sort

occurs when I direct my students' attention for the first time to the clearing up interval. They are quite insistent that there is nothing of the sort. But soon for them as for myself it stares at one everywhere.

I fear that these explanations rob our problem of most of its interest and mystery, but I think that they have some advantage in the direction of probability.

PROCEEDINGS OF THE MEETING OF THE NORTH CENTRAL SECTION OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION.

REPORT OF THE SECRETARY.

A meeting of the North Central Section of the American Psychological Association was held at the University of Chicago, April 22, 1905. About forty-five persons attended the meeting. The following papers, presented by members of the association and others, were read and discussed:

ABSTRACTS OF PAPERS.

The Perception of Reality. J. D. STOOPS.

Perception is the apprehension of a particular thing immediately present in experience. But because this particular thing is perceived as what it is, there is a 'fringe' about it, an ideal linkage which connects the particular with other things of its kind. If this ideal element, the relation, is abstracted and generalized, so that it is independent of the particular, we have a concept; if it is a 'fringe' about some definite concrete particular, we have a percept. In the process of perception we do not transcend experience. But when we conceive things, when we know things, do we not transcend experience? James says that we do not. Ideas are secondary, are dependent upon experience, and lead only to percepts. They never represent things beyond experience. Ideas stand therefore not between the mind and things external to the mind. They stand for some experience beyond immediate perceptual experience.

But if this be the true view, are we not driven to say that reality is only what we experience? It seems not. So far as this view goes, it leaves untouched the problem of the nature of things, in so far as they are not experienced.

The Irradiation of Light. FOSTER P. BOSWELL.

There are various modifications in the form of a small moving luminous image which are probably due to the presence of one form or another of visual irradiation.

There are five varieties of irradiation which may be conveniently distinguished but which are all capable of being brought under a single principle of explanation — the dissemination of neural excitation over the retina.

Irradiation a. — A very rapid spreading of the excitation over the retina, extending far beyond the borders of the stimulated portion and occurring immediately upon the impact of the stimulating light.

Irradiation β. — Irradiation within the stimulated portion of the retina after the form of a figure becomes distinctly perceptible.

Irradiation γ. — Emanations of decreasing intensity extend themselves outward and backward from a moving image until lost in the darkness of the background.

Irradiation δ. — The well known form of irradiation which occurs when a surface of greater intensity enlarges itself at the expense of one of less intensity.

Irradiation ε.—A form having many of the characteristics of *Irradiation a* but occurring only after long periods of stimulation, 30 seconds to a minute or more.

Report on Recent Work on the Growth of the Nervous System.
    HENRY H. DONALDSON.

The Wundt Pendulum Complication Apparatus as Tested by the Duddell Oscillograph. WALTER DILL SCOTT.

The Duddell oscillograph records times with perfect accuracy to less than a thousandth of a second. The Wundt pendulum complication apparatus contains within itself no device for testing its own accuracy, and many users of the instrument have felt themselves forced to abandon it. The instrument can be tested with perfect accuracy by means of the Duddell oscillograph. By such a test the instrument in the psychological laboratory of Northwestern University has been found to be subject to a constant determinable error. When the bell is adjusted to give the clearest ring and when the bell tap comes at the slowest movement of the hand the error is 27 thousandths of a second and is always negative.

Pragmatism and its Critics. ADDISON W. MOORE.

(This paper will be published in full in the May number of the *Philosophical Review.*)

Development of Ethical Sentiment in the Child. M. V. O'SHEA.

The infant reveals no true feeling of duty, no sense of ' the ought' in his conduct. Whatever action will bring him food and relief from distress is right, and may be freely performed. He shows no tendency toward self-restraint for the good of others in any of his activities. But by the twelfth week, perhaps, one may note the beginnings of the sense of a personal environment to be reckoned with. Before the completion of the first year the child will of his own volition restrain himself in his crying, teasing, bullying, appropriating the possessions

of his brothers and sisters, etc. He will also share his possessions with others. Ethical development thereafter, in all normal cases, is a long story of continually increasing self-restraint in certain social situations, and helpfulness in others.

The genesis of the child's self-restraint is found in his discovery that certain acts turn out ill for him; the social environment will not tolerate them. In a similar way he discovers that it pays to be helpful and generous. Social censure and approval are thus at the bottom of his ethical discriminations. But direct imitation reinforces tremendously the penalties and rewards administered by the social environment.

Until the inhibition or performance of an action becomes facile, so that it will occur more or less mechanically, there will always be something of a struggle between impulsions and restraining or importuning ideals of social origin, and this is felt as strain or tension, which we call the sense of duty in its simplest, crudest form. Actions at first *must*, then later *ought* to be performed.

Personal forces which in the beginning function in the child's consciousness focally and individually, come in the course of development to function subconsciously and coalescently. It may happen that all traces of concrete personal presence will disappear from consciousness; but this will not occur until conduct in any particular situation is so definitely established that to depart therefrom would occasion distress. Conscience arises slowly when social ideals begin to repress, modify, supplant impulsions; and it subsides when this work is thoroughly accomplished.

Feeling as Emotion and Sentiment: A Neglected Chapter in Psychology. Louis C. Monin.

The Upper Limit of Hearing as Affected by Differences in Age and Sex. Frank G. Bruner.

This paper presents some data on the upper limit of audibility as a result of tests made on several hundred Americans, varying in age from six to sixty-five years. These tests were made by the author, with the coöperation of Dr. R. S. Woodworth, at the Louisiana Purchase Exposition. The Edelmann type of Galton whistle was used in the tests, and afterward the pitch values of the various whistle lengths were carefully determined by objective methods.

Contrary to the results of Dr. Chas. H. Myer's tests on the Scottish children, some of the individuals tested were found to hear tones whose pitch values were upward of 40,000 v. s., and at least 50 per cent. of all whites hear tones as high as 31,000 v. s. The highest

tones are heard by those from 15 to 25 years of age. After this the upper range of hearing gradually declines, till at 65 years the average person hears with difficulty tones of 19,000 v. s.

Women on the whole have a greater range than men. In the earlier years this difference amounts at least to 2,000 v. s.

The Nature of Consistency. GUY A. TAWNEY.

Consistency was defined as (1) man's immediate sense of the self-maintaining quality of certain personal activities or (2) as the self-maintaining quality of these activities, the self-maintained being the social ego-alter self of reflection. Inconsistency, correspondingly, is the felt impossibility of reacting to a situation in self-maintaining ways. From this point of view the paper attempts to show that all criteria of value may be defined in terms of consistency and that the sense of consistency is simply one factor in the reflective type of consciousness, a contribution of reflection to the cognitive experience of the race. Types of consistency rest upon the three arcs of the psycho-physical process, viz., the arc of sensation, the arc of need, and the arc of movement or the production of change in the time and space order. Types of consistency are called presentative, practical and purposive. Presentative consistency is mechanical in form, the consistency of the merely presented data of judgments of value, a postulated consistency which is always constitutive in the world. Practical consistency is defined as that of voluntarily produced changes in the body or in other objects through the body. Purposive consistency is distinguished from the purposiveness of all consistency and indeed of all mental processes in being a reflective consciousness of the relation of means to ends. Three forms of this type appear in the harmony between objects and the mind's powers of apprehension, the harmony between an object and some end external to the object, and the harmony of the object with itself — beauty, utility and perfection.

WILLARD C. GORE,
Acting Secretary.

PSYCHOLOGICAL LITERATURE.

Einführung in die Psychologie. ALEXANDER PFÄNDER. Leipzig;
J. A. Barth, 1904. Pp. vi + 423.

This is an introduction in the strict sense of the word and not an
elementary treatise or outline. It discusses the general philosophical
or methodological questions preparatory to a treatise and does not pre-
tend to give details or even facts. The position taken on nearly every
question is that of the man on the street. The author takes a keen
delight in defining and tracing subtle distinctions that would do credit
to a scholastic, and he is very much more concerned with what must
be than with what actually is.

In the first chapter fifty pages are devoted to a definition of psy-
chology which contains nothing worthy of note. The second, the
material and the psychical reality, asserts that the physical and the
mental are equally real, independent and isolated from each other.
Interaction is supported against parallelism on grounds very similar to
those held by Busse. It is noteworthy that the author argues against
parallelism as if that doctrine asserted that mind and body could not in-
teract rather than that we did not know how they interact. The answer
to the argument from the doctrine of the conservation of energy is
met by the assertion that mind is probably some form of energy.
There is no recognition of the materialistic implications of this view.

In the third chapter we are told that introspection is the only psy-
chological method, although experiment is welcomed as an aid to in-
trospection. All objective methods are rejected with scorn. It is
denied that physiology, even sense physiology, can contribute anything
of value to psychology.

In the second part is given a discussion of the mental elements
and, very briefly, of mental laws. The whole system of mental ele-
ments is enormously complicated under the plea that the author is
avoiding epistemological assumptions. As a matter of fact he is sub-
stituting a very old epistemology for the newer. The primary mental
elements are sensation, feeling and effort (*Streben*). However, we
have not merely sensations on the intellectual side, but must distin-
guish in addition things, the aspects of things which may become
sensations (colors, tones, etc.) and sensation. When sensations are
to be known again they must themselves become objects of conscious-

ness, and then again mental states of the second degree. Whether the fact that we know that we know constitutes a third degree, or where the multiplication is to cease, we are not told. We are informed, moreover, that the consciousness of the object is entirely different from the object, and there is at least an implication that the qualities of the knowledge of the conscious states are absolutely different from the qualities of the conscious states themselves. The whole scheme is a reduction to absurdity of the popular tendency to multiply psychical entities beyond any necessity. Effort is made a third conscious element partly because the author finds it on analysis, but more because it is impossible to think the relation of subject to object without it.

The entire series of mental processes is further increased by the knowing self. This again is added not because there are facts to be classified but because we cannot think of the knowing process without the knower. Objects cannot know themselves. We cannot have unity in consciousness unless we have a unitary subject above consciousness. The argument for the necessity of a transcendental subject is not consistently carried out, for we are told in the section on self-consciousness that the self becomes an object of consciousness and is known by itself. An infinite regressus of subjects is not insisted upon. Apparently it is well enough to admit an impossibility if you can get it into small enough compass.

The treatment of attention on the analogy of a selection of different depths of perspective is well carried out and promises to be fruitful.

It is very remarkable that a professed psychologist should have been so little influenced by the thinking of the past century. Fortunately the attitude is not one that is likely to become general in a practical age.

W. B. PILLSBURY.

UNIVERSITY OF MICHIGAN.

Herbert Spencer. JOSIAH ROYCE. New York, Fox, Duffield & Co., 1904. Pp. 233.

This volume — for which the ' Autobiography' furnishes the occasion — contains a discussion of Mr. Spencer's contribution to the concept of evolution, a criticism of his theories of education, and a chapter of personal reminiscences by James Collier, for nine years the secretary and for ten years the amanuensis of Spencer. The reminiscences are interesting, but add nothing of moment to what is given in the ' Autobiography.' Mr. Spencer's opinions regarding education are so well known that there is little need of discussing them, except for the purpose of showing — as is very clearly done in this review —

that their singular onesidedness is due to the fact that they reflect his own individuality, and are a generalization from the method of training to which he was himself subjected.

The most important content of the book is the first paper. Professor Royce points out that Spencer's view of his own work was somewhat provincial, and that he was ignorant of the historical relationships of his doctrines. He did not understand that 'the great historical enemy of the evolutionary interest in philosophy has been not ' supernaturalism,' nor yet the doctrine of ' special creation,' but *the tendency to conceive the universe as an eternal, and so, temporally viewed, as an essentially permanent order.*' The proof that an alternative view has always, quite aside from Christian theology, existed along side of the evolutionary hypothesis is not presented in detail; it might be worth while, for the benefit of uninstructed readers, to show more specifically how large a part the conception of the eternity of the ' forms' of things has played, both in ancient and in modern thought.

Professor Royce would exempt the synthetic philosophy from some of the controversial tests which its opponents have sought to apply to it. He would not, for example, judge it on the basis of the theory of knowledge with which it is implicated. These discussions of knowledge and being expressed their author's ' limitation to certain very simple intuitions — the wholesome, straightforward intuitions of an English Radical, who, having early seen that we *can* know about natural causation, but cannot know anything about theology, and that we *can* know our rights and our duties, but cannot make out what it is that interests some people in Plato, and in Kant, and in all such speculators — henceforth reflects upon ultimate problems only for the sake of bringing to sharp expression the beliefs that he never learned to question or to analyze.' In other words, the system should be treated, not as a philosophy, but as a systematization of science. Another exemption, which many would not be so ready to concede, is from responsibility for the application of formulas derived from material phenomena to the description of mental and moral processes. "Spencer's formula was intended to hold true of phenomena only." His "business, as a student of phenomena, was with ' mechanism,' in the general sense, rather than with ' teleology.'" "He ought not, therefore, to be condemned merely because he undertook to conceive evolution in mechanical terms. He would have been false to his first philosophical purpose if he had conceived it otherwise." Yet the question may be asked whether this restriction of view may not have led to the misinterpretation of some of the phenomena involved.

In his restatement of the Spencerian formula, Professor Royce exhibits its inadequacy to the task assigned to it. As an empirical description of changes observed to take place in every process of development, its accuracy and ingenuity must be admitted. But is it a principle of explanation, which can be applied, like the law of gravitation, deductively, in individual cases ? It is safe to assume that the ultimate verdict will coincide with that here rendered. " Spencer's theory of evolution does not determine the relations of the essential processes of evolution to one another, does not define their inner unity, and does not enable us to conceive a series of types of evolutionary processes in orderly relations to one another."

One need hardly ask better help toward a just estimate of the great career so lately closed than is afforded by this little book in which historical, biographical and critical insights are happily blended.

EDWARD H. GRIFFIN.

JOHNS HOPKINS UNIVERSITY.

HISTORY OF EDUCATION.

Die Geschichte der Erziehung in soziologischer Beleuchtung. PAUL BARTH. Vierteljahrschrift für wissenschaftliche Philosophie und Soziologie, 1903, XXVII., 57–80; 209–229.

"Education is the intellectual propagation of society." The physical propagation of society furnishes the material for education. The family is not only society's special organ for its own perpetuation, but in all stages of culture is, and has ever been, the means by which its first steps in education are taken. Among primitive peoples there is no educational organization outside the family or the group to which the family belongs. The physical and the intellectual factors in education are inseparable.

Changes in the social organization, such as the almost infinite division of labor, the growth of ranks and classes, demand, and in general are accompanied by, changes both in the form and content of education. This truth the author develops and illustrates by a study of the primitive American, Asiatic and Egyptian peoples. Society and education act and react upon each other.

The content of education is capable of the following psychological division: (1) It must relate to the will and create (*a*) an attitude or disposition worthful to society by the implanting of the personal and social virtues, (*b*) a certain readiness or skill in the control of objects by the will — an essential to all knowledge. (2) It must influence the ideas of life (*a*) by handing down to succeeding generations the

results of individual thought, and (*b*) by conveying to them an accurate and comprehensive world view. These phases of education the author designates respectively by Zucht, Unterweisung, Unterricht and Belehrung.

· The training of the will is the most important part of primitive education. It is effected primarily by training in war, also by submission to the will of the chief and of the gods, by direct teaching of valor and endurance, and by reverence for parents. The fisher and hunter lack these virtues of the will because they have no fixed and permanent possessions in defense of which they must fight. In some measure they are possessed by the graziers and by the ruder agriculturists, but are seen in their highest form in the husbandmen of patriarchal peoples.

The author, having dealt mainly with the forms of earlier social life and education, promises to write on the relation of present day education to that of modern Europe.

HERBERT MARTIN.

YALE UNIVERSITY.

EXPERIMENTAL METHODS.

Die Massmethoden der experimentellen Psychologie. G. F. LIPPS. Archiv f. d. Ges. Psychol., 1904, III., 153–243.

After an introduction on the beginnings of experimental psychology, the author discusses the nature of psychical measurement, indicates the problem of psychology, and then passes to a consideration of the methods based upon measurement and enumeration.

Four principles are laid down for the development of the methods:

I. The first is that the measurements made on the basis of the connection of the physical and psychical possess not absolute, but variously conditioned significance, assignable only with reference to the prevailing influences.

II. Secondly, each observation refers to an interval of quantitative values.

Each observation is made under the influence of various factors, but we may, with repeated trials, make a distinction among these. There are (*a*) variable and (*b*) constant influences. The discrimination limen is to be considered a relatively fixed value, and is determined by the constant influences. The errors of observation are referable to the variable influences, whose effect is also increased or decreased by decrease or increase of power of discrimination.

III. Hence, the scattering of the estimations in a series is condi-

tioned by the constant influences, which determine an interval of values, as also by the variable, which act as disturbing factors.

Since one cannot know how the constant and variable influences together determine the series (IV.), the evaluation of a series of observations cannot be limited at the start by the acceptance of laws to which the scattering of the observations must be subservient. In particular it is unsatisfactory to use only the average error as a determination of a series, and unallowable to presuppose the ordinary law of error as a norm for the scattering of the observation-values.

After some discussion of the methods of Fechner and reference to later changes, the author concludes that, although, with the modification of the method of right and wrong cases which Bruns has given, we hardly have to consider the influence of the form of the law on the result, yet the difficulties in evaluation of the results, which a general law involves, make it not superfluous to attempt a development of the measurement methods without positing a law of error.

We may, perhaps, get the meaning of what follows thus:

Let the range of the measurements be divided into intervals $a_1 \pm l$, $a_2 \pm l$, $\cdots a_v \pm l$, the a-value representing the middle of the interval. The value of the norm for the observer, under the variable in connection with the constant influences, may be considered to fall a certain number of times in each interval. Represent these times by z_1^l, z_2^l, $\cdots z_v^l$. The total number of observations $= m = z_1^l + z_2^l + \cdots + z_v^l$. Let i be the discrimination limen, so that, if the apparent norm falls in the range $a_x \pm i$, we shall get a judgment of 'equal' when a_x is compared with the norm. Suppose a_x and the given norm are compared m times, and the number of 'equal' will be represented by z_x^i, and x may have values $1, 2, \cdots v$.

Now, $z_x^i : z_x^l = i : l$.

So, $\qquad (z_1^i + z_2^i + \cdots + z_v^i) : (z_1^l + z_2^l + \cdots + z_v^l) = i : l.$

$$i = \frac{l}{m}(z_1^i + z_2^i + \cdots + z_v^i).$$

If we write merely z_x for z_x^l, and take b as the point of departure or origin for the measurements,

$$E_x = \sqrt[x]{\frac{1}{m}\{z_1(a_1 - b)^x + z_2(a_2 - b)^x + \cdots + z_v(a_v - b)^x\}}$$

will represent the mean value of order x of the series. Then,

$$m = z_1 + z_2 + \cdots + z_v.$$

$$mE_1 = z_1(a_1 - b) + z_2(a_2 - b) + \cdots + z_v(a_v - b).$$

$$mE_2^2 = z_1(a_1 - b)^2 + z_2(a_2 - b)^2 + \cdots + z_v(a_v - b)^2.$$

. . . .

$$mE_{v-1}^{v-1} = z_1(a_1 - b)^{v-1} + z_2(a_2 - b)^{v-1} + \cdots + z_v(a_v - b)^{v-1}.$$

By these equations, any value of z_x, and therefore the course of the z-values, is determined.

If η_x represents the mean value of order x for b' as starting point,

$$E_x^x = \eta_x^x - \frac{x}{1}(b - b')\eta_{x-1}^{x-1} + \frac{x(x-1)}{\underline{|2}}(b-b')^2\eta_{x-2}^{x-2} - \cdots \pm (b-b')^x.$$

These mean values may be considered generalizations of the arithmetical mean and the mean error. For E_1 determines the arithmetical mean, since, when b is the arithmetical mean

$$\frac{z_1 a_1 + z_2 a_2 + \cdots + z_v a_v}{m}, \; b = b' + \eta_1;$$

and E_2 is the mean error.

Represent g series of observations by $a_{\lambda 1}, a_{\lambda 2}, \cdots a_{\lambda v}$ and $z_{\lambda 1}, z_{\lambda 2}, \cdots z_{\lambda v}$, where $\lambda = 1, 2, \cdots g$; $m_\lambda = z_{\lambda 1} + z_{\lambda 2} + \cdots + z_{\lambda v}$. The points of departure b_λ are the arithmetical means of each series. Suppose the series similar in all respects, so that they may be reduced to each other by increase or decrease of the observed values by the same amount. Then $m_1 = m_2 = m_3 = \cdots = m_g$, and $z_{1x} = z_{2x} = \cdots = z_{gx}$ and η_x will in general represent the mean values; if all the series be taken as one group of m observations and E_x represent the mean values of the whole, we shall get

$$gE_x^x = g\eta_x^x + \frac{x(x-1)}{\underline{|2}}\eta_{x-2}^{x-2}\Sigma(b_\lambda - b)^2 + \cdots$$

$$+ \frac{x(x-1)}{\underline{|2}}\eta_2^2\Sigma(b_\lambda - b)^{x-2} + \Sigma(b_\lambda - b)^x.$$

Now, in a given series of observations with arithmetical mean b and interval $b \pm i$, suppose this divided into g components with arithmetical means $b_1, b_2, \cdots b_g$, and intervals $(b_1 \pm d), (b_2 \pm d), \cdots (b_g \pm d)$, where $d = i/g$. So $b_1 = b - (g-1)d$; $b_2 = b - (g-3)d$; $\cdots b_g = b + (g-1)d$.

$\Sigma(b_\lambda - b)^{2\mu - 1} = 0$. And, since g may be taken as large as we please,

$$\Sigma(b_\lambda - b)^{2\mu} = \frac{i^{2\mu} \cdot g}{2\mu + 1}.$$

So

$$E_z{}^2 = \eta_z{}^2 + \frac{x(x-1)}{\underline{\lvert 2}}\, \eta_{x-2}^{x-2} \cdot \frac{i^2}{3}$$
$$+ \frac{x(x-1)(x-2)(x-3)}{\underline{\lvert 4}}\, \eta_{x-4}^{x-4} \cdot \frac{i^4}{5} + \cdots$$
$$E_2{}^2 = \eta_2{}^2 + \frac{i^2}{3},$$
$$E_4{}^4 = \eta_4{}^4 + 2\eta_4{}^2 i^2 + \frac{i^4}{5}.$$

If we now have n series of observations belonging together, established under like circumstances, so that η_2 and η_4 are the same for all, while the discrimination limena i_1, i_2, $\cdots i_n$ vary, we shall get $2n$ equations with $n + 2$ unknowns, which may then be determined. But it is questionable whether η_2 and η_4, which depend upon the distribution under variable influences, remain the same.

<div align="right">JOHN F. SHEPARD.</div>

UNIVERSITY OF MICHIGAN.

INTENSITY.

(1) *Definitions of Intensity.* (2) *A Study of Intensive Facts.* W. H. SHELDON. J. of Philos., Psychol. and Sci. Methods, 1904, I., Nos. 9 and 10.

Finding in existing definitions of intensity contradictions which can only be due to a misunderstanding of the meaning of the term, Dr. Sheldon has undertaken to find a new definition, which shall reconcile the differences of the old, and provide a criterion by which intensive facts may be recognized as such. In comparing the various definitions given by psychologists and physicists, he finds agreement in that intensity (by which he always means intensive quantity) is held to imply greater, less, or equal. But from this characteristic follow contradictions. For the property of being greater, less, or equal seems to imply measurability, and measurability seems to imply parts. But psychologists agree that no parts are distinguishable in intensity. Those of them who insist on the measurability of intensity assert that parts must be present, even though not to be discovered; while those who maintain its simplicity deny that it can be measured. (Yet later Dr. Sheldon states that 'psychical intensities certainly are measured.') Physicists find intensities capable of being measured, and yet incapable of being superposed or added, although superposition and addition have always been supposed to be conditions of actual measurement. The question then is, 'Do greater and less

imply parts, or can there be a kind of quantity which is logically incapable of addition or division or the properties usually associated with measurement?'

Modern mathematics offers a solution to the problem in its statement that it is possible to define a series ' each member of which may be greater than the one before . . . but such that there will be no whole-part relation unless you assume the associative and commutative laws.' Since intensities, unlike qualities, are capable of direct serial arrangement, we have in them an instance of such a series, provided that they are not subject to the associative and commutative laws, that is, provided that the series is not coexistent and reversible. The new definition which has now been found, that ' intensities are characterized by greater, less, or equal, and by absence of the whole-part relation, because they are describable in terms of order only,' seems to reconcile the contradictions of previous definitions. It still remains to find the essential property in intensity which ' allows it magnitude and denies it the whole-part relation.'

Taking time as an example of intensive quantity, Dr. Sheldon proves that in it we have a series that is essentially non-coexistent and irreversible, that is, transitive; since in time-measurements we are concerned only with the number of repetitions and not with the length of the repetitions themselves. (At this point, it is not perfectly clear what is meant by ' time-length,' although one judges that ' succession ' rather than ' duration' is meant.) Since the commutative and associative laws are applicable only to a coexistent, reversible series, it is evident that in time we have a genuine intensity. But since this non-coexistence or transitiveness which prevents the whole-part relation, is the essential property of the time-series, the temporal order itself may be regarded as the criterion by which intensities may be tested to discover whether or no they may come under this new definition, that ' intensities have the property of being greater, less or equal, but without the whole-part relation.'　　　　　　ETHEL BOWMAN.

WELLESLEY COLLEGE.

VISION.

Ueber das Verhalten der Netzhautzapfen bei Dunkeladaptation des Auges. W. A. NAGEL u. K. L. SCHAEFER. Zeitsch. f. Psy. u. Physiol. d. Sinn., 1904, XXXIV., 271–284.
Ueber den Einfluss der Dunkeladaptation auf die specifische Farbenschwelle. LOESER. *Ibid.*, 1904, XXXVI., 1–18.

Investigators have repeatedly demonstrated that dark-adaptation increases the brightness-sensitivity of the excentric regions of the

retina, — the most recent determinations by Piper showing that the
increase may amount to several thousand-fold. Since this phenom-
enon has been referred to a hyperæsthesia of the rods, the question
naturally arises: Do the cones also become more sensitive (to color)
when light is excluded from the eye? This is the problem which has
been attacked by Nagel and Schaefer in the present investigation.
Their experiments were confined to the fovea, and their method con-
sisted in obtaining successive determinations of liminal color stimulus
for different stages of a progressive dark-adaptation. It was found
that the sensitivity of the cones to red, to green and to blue increased
four-fold during the progress of the experiment. No determinations
were made for the initial stage of dark-adaptation, *i. e.*, the measure-
ments did not begin until the first minute of incipient dark-adaptation
had elapsed; but the authors infer that the total increase of cone-sensi-
tivity from the initial stage to the optimal stage of dark-adaptation
may be about 20-fold. In a second series of experiments, a red light
(visual angle of 20° to 30°) was thrown upon the foveal and para-
central regions. Here it was found that the amount of increase of
color sensitivity was much greater than in the former case. From the
end of the first half-minute to the end of the sixth minute the stim-
ulus limen decreased by $\frac{15}{16}$. It is inferred that when the stage of real
twilight vision has been reached, the color sensitivity of paracentral
regions has increased to about 200 times its initial value.

Loeser also was concerned with an investigation of the dependency
of color limina upon the condition of dark-adaptation. His stimulus-
object consisted of a colored and a colorless square of approximately
equal brightness, — the gray light being introduced for comparison,
and refinement of judgment. Each square subtended a visual angle of
13°; the whole stimulus-object an angle of approximately 28°. As
he puts it, the image fell upon ' macular, paracentral and peripheral
regions.' He employed a modified form of Piper's apparatus, and a
method which consisted in determining the liminal color stimulus for
different degrees of dark-adaptation. Loeser found that during the
' first minute' of dark-adaptation the stimulus appeared not only col-
ored, but well saturated. In no case did he establish an initial interval
of colorlessness. The color-limen was high during the early stages of
dark adaptation, but sank rapidly during the first few (6–12) minutes,
and gradually rose again at a later stage (20–46 minutes), after which
it remained constant. The variation of limen was different for differ-
ent colors. Sensitivity to red and to blue increased about 13-fold,
and to green 18-fold, while the subsequent decrease was very much

greater for blue than for green, and for green than for red. In the
absence of initial determinations, it is impossible to state the total
amount of increase, but Loeser conjectures that in the optimal stage of
dark-adaptation, color-sensitivity is 500 or 600 times greater than in
light-adaptation. Loeser does not agree with previous investigators
who have found that the adaptive change of sensitivity is in a constant
direction. On the contrary, he holds that the optimal stage is reached
relatively early, and that a decrease of sensitivity then ensues. He
suggests that this change of direction may be due to the heightened
sensitivity of the rods which may be assumed to contribute a whitish-
ness to the color sensation; and he points out that there is an approx-
imate coincidence between the point of time at which the color-limen
begins to rise, and that at which Piper found the brightness-limen to
begin its rapid descent. This view receives support from the fact that
there was scarcely any diminution of sensitivity in the case of the red
stimulus.

In the opinion of the reviewer, Loeser's experiments do not furnish
indubitable evidence of the presence in dark-adaptation of a diminu-
tion of sensitivity. His numerical results lack uniformity; different
determinations of the same limen (presumably by the same observer)
vary by as much as 45 per cent., and when the limina of different
observers are plotted, one finds curves which have little in common
with each other. These facts lead one to suspect the influence of a
factor which has not been taken into account. Loeser's description of
his procedure is unfortunately too meager to furnish a definite clue to
the solution of the difficulty. It seems probable, however, that there
may have been a progressive change of chromatic adaptation during
each series of experiments. At any rate, determinations of color-lim-
ina must, in the nature of the case, be less reliable when made serially
than when each is the product of a separate sitting.

<div align="right">J. W. Baird.</div>

Johns Hopkins University.

*Beitrag zur Lehre des intermittierenden Lichtreizes der gesunden
     und der kranken Retina*. E. P. Braunstein. Zeitschrift für
     Psychol. und Physiol. der Sinn., XXXIII.; 171–206, 241–288.

Braunstein's problem is to find the rate of succession of retinal
stimuli that is necessary for the production of a single sensory image.
Upon the determination of this rate depend greater accuracy in
medical diagnosis and the answers to many questions concerning
after-images and other visual phenomena. The solution of this
problem has often been attempted, but the results have been con-

tradictory. The author makes an effort to discover the causes of
their divergence.

In most of his experiments, the observer looks through an opening
in a sheet of black cardboard or through a small tube at a rotating
disk. This disk is made up of two, four, eight, or more alternating
black and white sectors. Each revolution of the disk is recorded on
the drum of a kymograph. This record is accompanied by a time
curve. The moment the increase of their speed makes the sectors
blend into an even gray, the observer breaks the electric circuit, by
means of which the entire group of apparatus is propelled.

According to Braunstein, those who have found that increase of
illumination hinders blending, and, therefore, requires a more rapid
rate, have failed to see that greater intensities of light heighten the
contrasts between the two stimuli. If the stimuli are such that their
contrasts remain unchanged when the illumination is increased, a
slower rate will produce fusion. He demonstrates this by means of a
disk, A, one half of which is gray and the other half white; and
another disk, B, one half of which is gray and the other half black.
The gray in both cases is the same as that which would be produced
by rotating a disk composed of the white and the black semicircles.
Let the illumination of the white $= 1$, and of the black $= 0$. The
illumination of $A = (1 + \frac{1}{2})/2$, or $\frac{3}{4}$, and of $B = (0 + \frac{1}{2})/2$, or $\frac{1}{4}$.
Therefore, A is illuminated three times as much as B. Nevertheless,
A blends with a slower rate of rotation than B.

The results depend, also, upon whether the eye has become accus-
tomed to the intensity of light used in the experiment. He finds that
the fovea requires a more rapid rate of succession for fusion after a
half hour's perfect rest in a dark room.

When the sectors are increased in number, more stimuli per sec-
ond are necessary. But the required rate of succession, he discovers,
remains more nearly the same the narrower the opening through
which the disk is seen. Diminishing the width of this opening makes
it less and less possible for the eye to follow the rotation of the sectors.
This is interpreted to mean that the increase of the rate with the
greater number of sectors is due, chiefly, if not entirely, to the greater
ease with which the eye can fixate and follow an object that makes a
small retinal image.

Braunstein asserts that all other investigators have overlooked
the fact that unless the eye is in a perfectly healthy condition, its
discriminative power is impaired. Hence, fusion takes place with a
slower rate of stimulation. He arrives at this conclusion through

the comparison of results from a large number of normal and defective eyes.

C. HOMER BEAN.

UNIVERSITY OF CHICAGO.

HEARING.

Studies in Pitch Discrimination. GUY MONTROSE WHIPPLE. Amer. J. of Psychol., 1903, XIV., 289–309. Commemoration Number.

This paper is a further working out of a problem treated by Dr. Whipple in two earlier papers, *i. e.*, the process of pitch memory and pitch discrimination as conditioned by individual observers and by varying time intervals. The first test was made on a young woman with some musical training, who at the age of twelve had discovered in herself the gift of absolute pitch. Her parents were unmusical but they came of musical families, so it is inferred that this gift may be an inherited musical *Anlage*, somewhat perfected by practice. This subject was not an experienced introspectionist. The results of the experiment were as follows: (1) Relatively accurate judgments of pitch can be made of instrumental or vocal music, the latter being more difficult. (2) Judgments are always direct, rapid, and in terms of visual-motor imagery, and referred to the piano key-board. (3) With no error greater than a tone, usually only a half-tone, under best experimental conditions, the piano notes were correctly identified ninety-two times in one hundred. (4) Accuracy of recognition is diminished if the clang-color of the notes is unfamiliar, *i. e.*, more correct judgments were made if the subject's own piano were used. (5) The octave in which a note lies is never mistaken. (6) Recognition is most accurate in the once and twice accented octaves. (7) With but fair ability to recognize the pitch of notes, this subject (*vs.* the generalization of Abraham) was able to image and reproduce assigned pitches very correctly. (8) Subject's sensible discrimination is no better than that of a trained musical observer, and is not aided by absolute pitch memory. (9) Kämpfe's method of modification of right and wrong cases was found to be the only satisfactory one with an observer of subjective type, *i. e.*, one whose judgments are easily modified by suggestion.

The subject in the second series of tests was a good introspectionist, a woman of almost no musical ability although fond of music. Dr. Whipple summarizes these tests of discrimination as follows:

"We have found that a typical unmusical observer, when placed

under proper conditions, may discriminate pitch differences of less than three vibrations correctly in 75 per cent. of the tests; but if the stimuli are of relatively low pitch, if they are given without any preliminary 'warming up,' if the time-interval between them exceeds four or five seconds, if they are given too briefly or in too quick succession, if they are of unequal intensity, or if they are presented simultaneously with one or more other similar stimuli, then discrimination becomes either difficult or quite impossible, and it may then remain impossible when D is represented not by a few vibrations, but by musical intervals of one or two octaves or more."

The third set of tests dealt with memory and pitch discrimination of chords and melodies. The subject, Professor I. Madison Bentley, was a trained musical observer. "As tested on the piano with half-tone intervals in the small octaves, with a time-interval of forty seconds and with distractions, the pitch of a chord is more difficult to remember and to discriminate than the pitch of a single clang. As tested under the same conditions, the pitch of a simple melody is as easily remembered and discriminated as the pitch of a single clang, possibly more easily. In the latter case, it is not clear whether the increased facility is due to the melodic form itself or merely to the greater number of stimuli employed." This last is one of the suggested problems which the article leaves to be solved.

The second subject said that certain notes seemed round, others triangular, etc. "These form-associations may point to a general tendency, with unmusical individuals, to transfer musical perceptions from auditory into other modalities" — a second point to be investigated. Dr. Whipple asks the further question: "Can musical incapacity, when discovered in childhood, be remedied by proper training?"

<div align="right">C. JEAN WEIDENSALL.</div>

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SPACE PERCEPTION.

Fresh Light on Molyneux' Problem — Dr. Ramsay's Case. T. K. ABBOTT. Mind, N. S., 1904, XIII., 543–554.

Dr. Abbott first gives a brief review of the discussion which centers around the famous query of Molyneux to Locke, whether a man born blind and made to see could distinguish a sphere from a cube by sight. Heretofore no direct answer has been obtained experimentally, but Dr. Abbott goes over the most important cases of operations for congenital cataract and points out their bearing on the theory of visual space perception. Dr. Franz did not present the question to his

patient in just the right form, and so it is of peculiar interest to find that the patient of Dr. Ramsay when shown a ball and a toy brick was able to tell which was which. This case was reported by Dr. Ramsay in the *Lancet* of May, 1903, and in a pamphlet now out of print. Dr. Abbott gets his information from Dr. Ramsay. The same case is reported independently by Prof. Latta in the *British Journal of Psychology* for June, 1904, vol. I., No. 2. The question in this case was not put until after the patient had seen a number of objects, but both Dr. Ramsay and the patient himself felt that the distinction was made by comparing what was seen with an imaginary tactile impression. This man, Carruth by name, recognized the first human face which he saw, first recognizing the mouth because the sound of the voice came from it. He also recognized that the fenestrated pattern of a bench back was similar to an ' arch ' but not so flat. Prof. Latta's own observations on this point differ materially from the version given by Dr. Abbott.

Carruth had a distinct visual perception of distance and magnitude, although his estimates were not at first accurate. All these facts are largely confirmatory of the theory of visual space perception maintained by Dr. Abbott in his *Sight and Touch*. The case is a valuable addition to the mass of evidence which he has accumulated. He notes as significant, also, the fact that we have here an exception to the general rule that the acquisition of sight after such an operation is a slow and laborious process; evidently it is not necessarily so.

WARNER BROWN.

UNIVERSITY OF CALIFORNIA.

*Experimentelle Untersuchungen der beim Nachzeichnen von Strec-
ken und Winkeln entstehenden Grössenfehler.* J. RICHTER u.
H. WAMSER. Zeitsch. f. Psych. u. Phys. d. Sinnesorgane, 1904,
XXXV., 321–339.

Is there any uniformity in the errors occurring in the reproduction of simple geometrical figures? The method was that of reproduction by free hand drawing. The standard figures were lines 5 mm., 10 mm., 50 mm. and 100 mm. long; and angles 30°, 60°, 120° and 150°, each in three positions with the vertex either at the right, at the left, or at the top. The lines 5 mm. and 10 mm. long were overestimated, *i. e.*, were drawn longer than the standard, and those 50 mm. and 100 mm. long were underestimated in the reproductions. All angles except 120° which had one leg horizontal were underestimated. Of those which had the vertex at the top 30° and 60° were overestimated and 120° and 150° were underestimated. These experiments

were made by Richter. Wamser repeated them with some changes. The results agree in almost all respects. No explanation of the results is suggested.

<div align="right">DANIEL STARCH.</div>

UNIVERSITY OF IOWA.

READING.

Zur Psychologie des Lesens bei Kindern und Erwachsenen. O. MESSMER. Archiv f. d. Ges. Psychol., 1903, II., 190–298.

In view of the disagreement between recent experimental studies of the psychology of reading, the present paper exploits the influence of individual variations and initiates a comparative study of the reading processes. On the basis of the tachistoscopic experience of four adults and six children, from seven to eleven years old, Messmer distinguishes two types of readers, the objective and the subjective.

The former is characterized chiefly by the closest correspondence between the physiological fixation point and the point of attention. The limited fluctuation of the attention permits the apprehension of only a very limited number of letters at each exposure. A long word is read only after a number of successive exposures in which the attention is successively fixed on small groups of letters. There are few mistakes, but only comparatively little is read. The subjective type is characterized by a fluctuating or wandering attention (even during an exposure of 2 σ duration!). The field of attention is relatively large. A whole word may be read from an eccentric fixation point. Attention is directed inward to the words at hand, and there is much confusion as to what is observed and what is interpreted. The objective type is best exemplified by the performance of a trained observer. The subjective type is more common both in adults and in children.

The author's discussion of the word form, one of the most difficult of our present problems, is dogmatic, careless in its generalizations and indifferent to the experimental data already at hand.

Far more significant is a new hypothesis of the successive apprehension of letter groups. In contradistinction from Zeitler's left to right succession, Messmer's succession hypothesis depends on the difference in the optical distinctness of letters and letter groups. The dominant letters are more easily and hence more quickly perceived. Letters and letter groups which are less distinct come to consciousness later.

Grammatical categories and orthographic rules are practically ignored by children in tachistoscopic reading. The effects of fatigue are found to be greatest in the most rapid reading. They are more

marked in children than in adults. Children read nonsense texts
almost as rapidly as significant texts. Adults read the latter in half
the time of the former. A valuable collection of misreadings is
given at the end of the paper, grouped with reference to their kind
and the frequency of their occurrence with children and adults. The
reviewer still ventures to doubt that the results of minimum exposure
and threshold stimulation may be transferred bodily to the processes of
normal reading without doing violence to the facts.

<div align="right">RAYMOND DODGE.</div>

WESLEYAN UNIVERSITY.

FATIGUE.

*A Study of the Accuracy of the Present Methods of Testing Fa-
tigue.* A. CASWELL ELLIS and MAUD MARGARET SHIPE.
Amer. J. of Psychol., XIV., 496–509 (Commemorative Number,
pp. 232–245).

A variety of methods for testing fatigue were employed to see
whether when used at the same time on the same subjects they would
show corroborative results. Five students and one professor were
tested two or three times a day for five days, using more than one
method of testing at each period. The methods used were : reaction
time for recognition of familiar names used with the ergograph — 27
perfect tests; reaction time (as before), addition of columns of figures,
writing the cubes of numbers up to 9, memorizing nonsense syllables,
all used together — 24 perfect tests.

A year later seven subjects were employed on the same tests with
the addition of Ebbinghaus' method of filling in blanks made by
omitting letters and small words from printed matter. There were
14 perfect records taken, and 24 more in which the reaction test was
varied. With a view to trying the effect of testing untrained as com-
pared with trained minds, ten children from 11 to 15 years of age
were tested for three days. Adding, cubing, learning nonsense syl-
lables, and filling blanks were the tests used.

These tests were taken at such times of day as would be likely to
show a contrast between a rested and fatigued condition, as at 9 a. m.
and 5 p. m. During some of the time such exhausting work as that
of taking examinations came between them.

The results of these tests failed to show any distinct amount of
agreement in the indication of fatigue. In the case of the reaction-
time-ergograph test, on only 6 out of 27 times did an agreement occur
between the reaction and ergograph records, while about half the results

indicated greater power when the subjects were really fatigued. With the larger combination of tests which followed, the reaction time and variations increased or decreased together 13 out of the 24 times, the reaction agreed with the addition test 13 times, with the cubing test 10 times, with nonsense syllable test 6 times, etc. The other results were of a similar nature. Other ergograph tests which were taken to measure the motor energy were also unsatisfactory. The conclusion of the authors is that these tests as now used are worthless.

It is unfortunate that the editors were obliged to cut out the tables which accompanied the original manuscript of this article, as these might have given some cue to positive results. There are, however, certain conditions which could not but help make a confusion in the results. In the first place fatigue is not simple as assumed by this study, but complex. When one has worked and produced even an extreme feeling of fatigue it is often through the exercise of but a comparatively few nerve elements or muscles. Others may be in a high state of efficiency. The test should be adapted to the *kind* of fatigue, otherwise the result will be as confusing as would be the testing of a town's wealth by examining the pockets of a chance citizen. There is also a motor excitement which accompanies some forms of fatigue and causes an acceleration of rate in the working of another function, *e. g.*, adding is accelerated after a fatiguing walk or after learning nonsense syllables. Some effect of this sort is also likely to creep in when tests of different kinds quickly follow each other as in the case of these experiments. Finally, one needs to distinguish between fatigue and languor, as these may produce different effects and either may be present without the other.

J. P. Hylan.

Ueber Ermüdungskurven bei Gesunden und bei einigen Neurosen und Psychosen. H. Breukink. Journal f. Psych. u. Neurol., 1904, IV., 85–108.

The larger part of this article is an historical and critical résumé of previous investigations on fatigue. Dr. Breukink obtained fatigue curves with Kraepelin's ergograph. Weights of 3, 4, and 5 kg. were lifted repeatedly and at a constant rate with a finger until complete exhaustion was reached. The following are some of the characteristic results.

For normal health, the second was the maximum lift in the first record, while in the second record the fourth was the maximum lift. The number and the average height of the liftings is greater for the men than for the women.

For hysteria, the maximum lift is the third or fourth. Frequently a sudden drop occurs in the curves.

For neurasthenia, the number and the height of the liftings are less than for normal persons. The first lift is frequently the maximum lift.

For chorea, the curves fluctuate very irregularly.

For dementia hebephrenica, dementia epileptica, and dementia paralytica, the average height of the liftings is less but the number of the liftings is the same or greater than for normal persons.

These experiments seem to have been made with the assumption of undoubted reliability of ergographic tests, which has been seriously questioned by recent studies.   DANIEL STARCH.

UNIVERSITY OF IOWA.

MOVEMENT.

A Sketch of the History of Reflex Action in the Latter Half of the Nineteenth Century. RORERT H. GAULT. American Journal of Psychology, 1904, XV., 526–568.

For the sake of a clearer presentation than is possible under a broad generalization, the author has treated his subject under the following : ' Statement of the Theory and Extent of Knowledge at the Time of the Pflüger-Lotze Discussion, Inhibition of Reflexes, Phenomena of Summation, Vascular Tonus, Muscular Tonus, Tendon Reflexes, Direction of Transmission and Coördination of Reflexes, and Speculative Considerations.'

Each division is a comprehensive résumé of the experimental facts so far discovered as well as the theoretical conclusions reached. The author's individual estimate of the real contribution to knowledge is thus stated : " Reviewing the history of the fifty years with reference to progress in the theory of reflex action we see clearly that the main gain has been rather in the slow alteration of standpoints than in any sudden appearance of new facts wholly incompatible with older views. The spirit of the age — its unconscious metaphysics — has changed and along with it the metaphysics of the reflex."

WM. R. WRIGHT.

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Über Assoziationsreaktionen, die auf optische Reizworte erfolgen. HENRY J. WATT. Zeitsch. f. Psychol. u. Physiol. der Sinn., 1904, XXXVI., 417–430.

The reactions of eight subjects to familiar words of various classes, visually perceived, were timed by the chronoscope. As in the case of

Thumb's and Marbe's corresponding acoustic series, adjectives, pronouns, adverbs of place and time, names of relationship and numbers (versus Oertel) usually associated words in the same class — the exceptions ranging from 34 to 45 per cent. Oertel's discrepancy is ascribed (1) to allowing five seconds for the presentation and twenty for forming the association, whereas with the other investigators twenty seconds rarely elapsed before the subject reacted. This period is too long to allow accurate analysis of the mental experience. (2) To not controlling the reaction time but allowing secondary and tertiary associations, whereas the others ask for the first association.

When a word *a* associates *b*, *b* associates *a*, but numbers usually associate higher numbers, which is in harmony with the results for acoustic stimuli (Thumb-Marbe). The Marbe frequency law also holds for optical stimuli: the average reaction time diminishes with the increasing frequency of the word, first very rapidly, then slowly and finally unappreciably. But the optical stimuli give larger time differences. The mental speaking of the visual word did not influence the form of the association.

J. E. W. WALLIN.

PRINCETON UNIVERSITY.

BOOKS RECEIVED FROM MAY 5 TO JUNE 5.

L'Idealismo Moderno. G. VILLA. Torino, Fratelli Bocca, 1905. Pp. xiv + 452.

The Psychology of Beauty. E. D. PUFFER. Boston and New York, Houghton, Mifflin and Co., 1905. Pp. 286.

Les Mensonges du Caractère. FR. PAULHAN. Paris, Félix Alcan, 1905. Pp. 276. (Bibliothèque de Philosophie Contemporaine.)

Der Vitalismus als Geschichte und als Lehre. Dr. HANS DRIESCH. Leipzig, J. A. Barth, 1905. Pp. x + 246. (Natur- und kulturphilosophische Bibliothek. Band III.)

Die geistige Ueberbürdung in der modernen Kultur. MARIA VON MANACÉINE. Übersetzung, Bearbeitung und Anhang: *Die Überbürdung in der Schule.* Dr. LUDWIG WAGNER. (Natur- und kulturphilosophische Bibliothek. Band II.) Leipzig, J. A. Barth, 1905. Pp. v + 200.

L'Année Philosophique. F. PILLON. Paris, Félix Alcan, 1905. Pp. 316. (Bibliothèque de Philosophie Contemporaine.)

The Color Sensitivity of the Peripheral Retina. J. W. BAIRD. Washington, Published by the Carnegie Institution, 1905. Pp. 5 to 80.

Metaphysical Elements in Sociology. P. H. FOGEL. (Diss.) (Princeton Contributions to Philosophy, Vol. 1, No. 4. Repr. from the American Journal of Sociology, Vol. X., Nos. 3, 4, Dec.–Jan., 1904–5.

Beiträge zur Psychologie der Aussage, herausg. von L. WILLIAM STERN. II. Folge, 2. Heft. Leipzig, J. A. Barth, 1905. Pp. 154.

L'être subconscient. G. GELEY. 2e éd., revue. Paris, Félix Alcan, 1905. Pp. 176.

NOTES AND NEWS.

The following items are taken from the press:

Professor E. B. McGilvary has tendered his resignation from Cornell University, to take effect next year, when he will accept the chair of philosophy at the University of Wisconsin.

Professor James H. Tufts, of the University of Chicago, was elected president of the Western Philosophical Association at the meeting held at the University of Nebraska on April 21 and 22.

Dr. E. B. Holt has been appointed assistant professor of psychology at Harvard University.